Cadaveric Kidneys for Transplantation

SIR,—The timely article by Professor R. Y. Calne (31 May, p. 565) illustrates some of the problems associated with the supply of cadaver kidneys for patients in chronic renal failure. It is important to realize that cadaver renal transplantation is as an effective form of treatment of patients with chronic renal failure as any surgery for most of the common cancers. While transplantation or dialysis is available for less than 10% of the young patients dying of renal failure in this country, most of the donor organs can be harvested by the transplant teams. As facilities become available for more patients it will be necessary to enlist the help of surgeons throughout the country to remove kidneys from cadaver donors.

It is essential, therefore, to keep this operation as simple as possible so that it can be performed by surgeons in training in any hospital. The object of the donor operation is to remove both kidneys as soon as possible after death and to cool them. The kidneys, their vessels, and ureters must not be damaged during removal. When cool the kidneys must be kept sterile and must be

a pair of curved scissors up behind to the aortic hiatus to divide both I.V.C. and aorta. This causes the escape of a large quantity of blood, but the block of tissue required is by this time free. The block of tissue contains both kidneys and ureters, half both adrenals, perinephric fat, a small portion of pancreas, and long sections of the aorta and I.V.C. (Figure).

In contrast to the method described by Professor Calne this procedure does not require good lighting, as no fine dissection or ligaturing is necessary deep in the posterior abdomen. The block of tissue is transferred to a sterile surface, ventral surface down. The aorta is split longitudinally along its posterior wall to display the renal arteries. The block is turned over and the I.V.C. is opened similarly (Figure, line A-A). It is now a simple matter to divide the anterior aortic (Figure, B-B) and posterior vena cava walls and gently to pull apart the two kidneys with their vessels. The perinephric fat is opened to allow inspection of the renal surface. This method enables both kidneys to be removed in 15–20 minutes. Removing the kidneys serially as described by Calne in our hands almost doubles mean time from

Diagram to show the block of tissue removed from the corpse for dissection. The aorta and I.V.C. are divided along lines A-A and B-B, with care not to damage the right renal artery and left renal vein respectively.

easy to transport to the transplant recipient's hospital. We wish to describe our technique, which was developed after discussions with the transplant team at St. Mary's Hospital.

The abdomen is opened through a transverse incision. The peritoneum is incised lateral to the splenic flexure of the colon and the kidneys and perirenal tissues are mobilized from their lateral side. The ureter is identified at the pelvic brim and with the periureteric tissues it is mobilized well into the pelvis and upwards to the lower pole of the kidney. A hole is made in the colonic mesentery between the middle and left colic arteries, and the kidney and ureter are pushed medially through the hole. The hepatic flexure of the colon and caecum are mobilized and the incision is carried up across the aorta. The right kidney and ureter are mobilized in the same way as the left. The bowel is thrown up over the costal margin. The aorta and the inferior vena cava (I.V.C.) are divided between cholecystectomy clamps just above the aortic bifurcation. The left hand of the operator now grasps both kidneys, ureters, and aorta and I.V.C. with their clamps. With gentle downward traction the dissection is carried up posterior to the great vessels to the aortic diaphragmatic hiatus. The dissection is now carried up in front of the aorta and left renal vein behind the duodenum and pancreas to expose the superior mesenteric artery and coeliac axis, which are divided. It is now a simple matter to pass

starting the donor operation to the start of kidney cooling. We suggest that our method also has the advantage that it allows the surgeons performing the transplant to decide how to trim the donor vessels, thus resulting in the best possible technical fit.

The method of cooling described by Professor Calne appears rather formidable and may deter some surgeons. We find that preservation for 3–4 hours at 5° C. without infusion or perfusion of a kidney at the donor operation allows grafts to function satisfactorily. The kidneys are doubly wrapped in Vidrape or Aldon intestinal bags. The air is expressed, the tapes are tied tightly, and the bags are plunged into a bucket of ice and water. The closure of plastic bags must be performed carefully, as we have demonstrated the leakage of fluid through elastic band and plastic tape closure, with contamination of the kidney. By placing saline in the bag with a kidney it is difficult to detect contamination; when no fluid is placed in the bags any that gets in excludes the kidney from use.

We accept that Professor Calne has a wider experience than we with this operation, but we feel that our experience is more like that of surgeons who may be called upon in

the future to perform the operation. It is our purpose to try to increase the availability of kidneys with adequate ureters and vessels so that transplant surgeons should have the minimum of technical problems during their operation. Although organ preservation to allow planned recipient operations is ideal at present, none of the available methods is suited to widespread use. The method we use requires no special equipment or facilities.—I am, etc.,

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Sudden Death in a Young Asthmatic

SIR,—Deaths in asthmatics are always tragedies and it is excellent that a detailed report of such a case has been published by Dr. I. Gregg and Dr. J. Batten (5 April, p. 29). Nigh on 40 years in a large medical clinic (first in Lund, then in Uppsala) has given me some experience in the matter.¹

So far as I can see the remedy which is of paramount importance is potassium iodide, and in emergencies it may be given intravenously, just as in thyrotoxic crises. It serves to dissolve the tenacious mucus which is responsible for the suffocation. Adrenaline or one of its modern substitutes serves to relax the bronchial musculature, and soluble cortisone intravenously takes care of the allergic factor and the swelling of the mucous membranes. It goes without saying that the patient may have to be treated in a respirator.

Of the three remedies mentioned the iodide seems to me of paramount importance. On rare occasions when a death from bronchial asthma has occurred in my absence it has invariably been in cases in which potassium iodide has not been given.—I am, etc.,

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REFERENCE

- 1 Ask-Upmark, E., *Akut Medicin*, 1969, Almquist and Wiksell, Stockholm.

Cervical Cytology

SIR,—I was interested in the leading article (7 June, p. 585) which cast doubt upon the interpretation drawn by the workers in British Columbia¹ of their own data regarding cervical smears, where they have suggested a possible 50% spontaneous regression rate of carcinoma-in-situ. Our data in Aberdeen² show that the incidence of clinical carcinoma in our area corresponds with what would be expected from the observed detection rate of carcinoma-in-situ in a comprehensively screened population.

The annual incidence of clinical cervical carcinoma in the city of Aberdeen is 21 cases,³ there being 435 cases of clinical cervical carcinoma in the gynaecological wards during the 21 years 1946–66. According to the 1961 Census there were 60,000 married or formerly married women in the city, and the detection-rate of preclinical cervical cancer is 0.7%. With this detection rate a population of this size would be expected to yield 420 cases of preclinical

cervical carcinoma. These, if spread over the suggested period of 20 years for the development of the disease, would present at the rate of 21 cases every year—that is, precisely at the known annual incidence. It is therefore necessary for all the detected pre-clinical cases to progress to clinical cancer, if left untreated, to produce the expected number of clinical cases each year.

The incidence of the disease has dropped in screened populations, both here² and in British Columbia. The fall in mortality will follow because the precancerous lesions, which we believe would progress, have been removed after detection by screening techniques. In a disease with a 10–20 year latent period between the preclinical stage and death time must elapse until the results of screening can be reflected in mortality rates.

As we have said,² and as has Way,⁴ efforts must be concentrated on encouraging the screening of women of all ages. In Aberdeen young women are screened antenatally, their names put on a computerized register, and they are automatically recalled for screening at five-yearly intervals. The older women are screened in co-operation with their own general practitioners, and may be recalled at shorter intervals.—I am, etc.,

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REFERENCES

- ¹ Fidler, H. K., Boyes, D. A., and Worth, A. J., *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1968, **75**, 392.
- ² Macgregor, J. E., *Lancet*, 1967, **2**, 1296.
- ³ Aitken-Swan, J., and Baird, D., *British Journal of Cancer*, 1966, **20**, 624.
- ⁴ Way, S., *Lancet*, 1969, **1**, 1151.

SIR,—Your leading article "Do Cervical Smears Save Lives?" (7 June, p. 585) raises many interesting points, not least the spontaneous regression of preinvasive carcinoma of the cervix. As you state, regression hardly ever seems to have been observed. It is very difficult to prove if any surgical intervention has been used, but in the few patients in my clinic over the last 20 years who have refused treatment they have either proceeded to clinical cancer (one took 16 years to do so) or the smear has remained positive while under observation, and at least one has died of other causes without developing clinical carcinoma.

We have, however, seen three patients in whom spontaneous regression probably did take place. All three were pregnant when first found to have positive smears. One patient's smear reverted to negative after delivery, but became positive in each of two succeeding pregnancies, only to revert to negative after the third delivery. Hysterectomy was later performed and serial sectioning failed to reveal any abnormality in the cervix. A second patient is still being followed after reverting to a negative smear after positive smears in two pregnancies. The third is too recent for comment.

A likely explanation of this phenomenon would seem to be that while the patient is not pregnant her immune response is sufficient to combat successfully the malignant potentialities of her cervical epithelium, but when she is pregnant some substance appears to protect her foetus suffering host rejection and this substance also blocks the immune receptors that have previously dealt success-

fully with the cervical epithelium, and in these few cases after pregnancy normal antigen-antibody reaction takes place.—I am, etc.,

STANLEY WAY.

University of Newcastle upon Tyne.

Pulmonary Blood Flow

SIR,—May I comment on Dr. B. Towers's contribution (24 May, p. 509) to the debate on my paper on stratified pulmonary blood flow in the secondary lobule of the lung?

I have carefully read the paper¹ by Dr. Towers to which he referred in his letter; and I have noted his own assessment of it, given there, as presenting "a concept which is likely to revolutionize the field of respiration physiology." For all that, I cannot see what bearing the investigations, hypotheses, and comments that he records in that work have on my own paper. His studies relate to the foetal and immediately postnatal lung. They indicate that, if one cuts 5 mm. slices of such unfixed and unsupported lungs and drops them in fixative (the process of "real biopsy"), different parts of that lung appear differently expanded. I would not question this for a moment. But, whether one accepts his subsequent interpretations or not, they do not seem to undermine (or, for that matter, even to bear on) my own conclusions about stratification of blood flow within the secondary lobule.

The other theme running through Dr. Towers's letter (and the paper he refers us to) is that "pulmonary physiologists" are an extraordinarily homogeneous group, and a singularly imperceptive group at that, in not having recognized his essential truths. I know of no reputable "pulmonary physiologists" who "for many decades" have championed the assumptions that Dr. Towers ascribes to them as a group. In my own role (of which being a "pulmonary physiologist" forms, in turn, only one part), I have published upwards of 25 papers in the last decade specifically on the themes that inspired gases are *not* distributed in the way he suggests we assume, and that the pulmonary vascular bed is anything but a passive, "permanently open" circuit, accepting whatever the right ventricle tosses it in either health or disease. Over the same period a number of real "pulmonary physiologists" have made much larger contributions on the same themes. I really cannot see the "ultimate absurdity" of my views (or theirs) against this sort of background; it may be good polemics, but it is pretty poor science.

I should, of course, require a little more specific criticism of my functional studies extending over some years than their blanket dismissal (dare I say, "by an anatomist"?) as "a very few experiments and with inadequate control of the many variables" to wring my withers or, indeed, to be able to make a defence.

As to the teleological desirability of the brains being in the boots on hydrostatic grounds: well, I have heard it said of some. . . . —I am, etc.,

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Sydney, N.S.W., Australia.

REFERENCE

- ¹ Towers, B., in *The Biology of Gestation*, ed. N. Assali, 1968, vol. 2, pp. 189–223. New York, Academic Press.

Scalp Veins and Scalp Needles

SIR,—With reference to the paper from this institution by Dr. J. H. Darrell and Professor L. P. Garrod on secondary septicaemia from intravenous cannulae (24 May, p. 481) it is suggested in your leading article on the subject (24 May, p. 462) that "the use of small needles for infusion into scalp veins (our italics) may provide a satisfactory answer" and thus avoid the use of indwelling cannulae.

Lest there be any misunderstanding, we wish to make it clear that we do not treat our patients with infective endocarditis by infusion into their scalp veins.

We adopted the use of scalp vein needles because they are thin-walled and suitable for insertion into small veins with minimal trauma. We suggested changing the site of infusion every two days in order to avoid thrombus formation with its inherent dangers. The same forearm veins can be used many times, and immobilization of the limb is also unnecessary (Mr. A. H. Hunt, 7 June, p. 632).

We believe that this technique is effective and reduces the patient's discomfort to a minimum in what used to be a gruelling endurance test as well as a dangerous disease.

—We are, etc.,

CELIA OAKLEY.

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. We regret the ambiguity in the description of the needles.—ED., *B.M.J.*

Acute Infective Gastroenteritis

SIR,—I have read with interest the letter from Dr. Mary J. Wilmers and others (31 May, p. 573) concerning your leading article on acute infective gastroenteritis (3 May, p. 263). They make a pressing claim for the use of tetracycline or neomycin in all hospital admissions for gastroenteritis on the grounds that "epidemics could be arrested, dramatic clinical improvement occurred, and pathogenic *E. coli* disappeared from the stools." This department also deals with the disease, and I have not observed dramatic clinical benefits in cases treated with antibiotics compared with untreated cases, and all too often pathogenic *E. coli* fail to disappear from the stools, even after repeated courses of antibiotics to which the organisms are sensitive. Therefore I cannot share their optimistic view of the value of antibiotics in the treatment of the individual case.

Their recommendation of the blanket use of antibiotics for all cases throughout their stay in hospital and for several days after may be of value in the control of cross-infection, but this measure would need the support of scrupulous epidemiological trials before being generally acceptable, as it would have the disadvantage of tending to increase the level of resistant coliforms in the community.

Finally, Dr. Wilmer and her colleagues have chosen to support their argument by posing the question, "In what other bacterial infection with a high mortality would it be considered good medical practice *not* to administer the antibiotic to which the infecting organism is sensitive?" I would like to sug-